On Adhesion Figures

A DROP of crude carbolic acid placed on the surface of cold water exhibits the most surprising and beautiful "cohesion figures." I am not aware that Mr. Tomlinson has described the behaviour of this substance. So lively and unpredicted are the movements of the drop that its action resembles that of a living creature. At first it pulsates, then its edge breaks up into crispations, and a motion begins like the waving tentacles of a sea-anemone. Sometimes the drop will sail about in a crescent shape, or shoot out independent little rings, which gyrate and rush about like a rotifer, until at last they burst into a myriad of intensely active little specks. Warm water destroys all action, by lessening, I suppose, the adhesion of the liquids.

To your readers it is trite and useless to remark upon the interest that attaches to the careful and continued observation of the most familiar things. But I wish the leisure classes could understand this. How many pleasant and instructive hours might some of them spend in examining such common things as essential oil dropped on water, even drops of ink falling through water, or puffs of smoke through air or a candle flame, or a hydrogen flame, or iron filings sprinkled on paper over a magnet, or, among other simple things, best of all a block of ice in a sunbeam. From the contemplation of such phenomena one rises with an increasing joy, and not a little humbled at one's own ignorance before the orderly mystery that pervades everything.

24, Elgin Road, W. J. H. SPALDING

A Suggestion to Opticians

THERE is a method of mounting self-registering meteorological thermometers very commonly employed, and one which for some purposes, as, for instance, for determining the temperature immediately over the surface of the ground, is an excellent plan.

I allude to the placing of the divided thermometer stem in a larger tube forming a jacket to it, and fixing it with india-rubber packing, which makes an air-tight stopper round the neck.

Now it is frequently observed that when thermometers fitted in this manner are exposed to cold, a copious deposition of dew takes place, both on the stem and in the interior of the jacket, rendering the accurate reading of the instrument a matter of some difficulty. I would therefore suggest to makers of this class of instrument whether it would not be advisable for them to *dry*, I do not mean merely heat, the air in the tubes, or else enclose some water-absorbing substance, as calcium-chloride, in the tube before finally inserting the india-rubber stopper.

It is extremely probable my suggestion is not a new one. I have no recollection, however, of having heard of such a plan being employed by any maker. G. MATHUS WHIPPLE

Kew Observatory, June 10

The Ferrara Floods

MAY I ask for space to draw the attention of engineers to the question whether the beds of embanked rivers rise or not? The affirmative is argued (and in reference to Ferrara) in the last chapter of "Rain and Rivers," against the negative of the eminent American engineer Ellet. The title of the chapter is "Ellet on the Mississippi." GEORGE GREENWOOD

Brookwood Park, Alresford, June 8

FORCE AND ENERGY

THE CONSERVATION OF ENERGY A FACT, NOT A HERESY OF SCIENCE

IN an article entitled "The Heresies of Science," published in a recent number of the *London Quarterly Review*, two widely different principles are oddly linked together as heretical dogmas, the doctrine of Evolution, and the Conservation of Energy.

On the doctrine of *Evolution* the writer has nothing to say.

Before discussing with the "Reviewer" the validity of the *Conservation of Energy*, it is quite necessary to define the terms which may be employed, such as Force, Energy, Potential, Sound, Light, Heat.

It is much to be regretted that a far greater degree of logical accuracy in the use of terms than is usually met with, does not exist amongst even the ablest writers on physics, for many of the arguments adduced against physical principles lie not against the principles themselves, but against the indefinite language in which they have from time to time been expressed. There is probably no term employed in physics that has been more misapplied, and in its misuse has led to greater confusion of ideas, than "force."

Force has been thus defined by our ablest modern physicist.* "What I mean by the word force is the source or sources of all possible actions of the particles or materials of the universe."

This definition of force is substantially the same as the writer's definition † to which the reviewer takes exception, but which may perhaps with advantage be thus amplified: Force is a mutual action between the atoms or molecules of matter, by which they are either attracted towards, or repelled from, each other; and by this action energy is imparted to the matter put in motion. It may be further remarked that force is essentially either attractive or repulsive.

The writer sees no reason to amend his definition of "Energy"—namely, that it is *the power of doing work*. It may, however, be remarked that the existence of energy in matter implies the existence of motion, and *vice versâ;* but it by no means follows that motion and energy are convertible terms, for motion means only the act of moving, or changing the position occupied in space.

The term "Potential" applied to force or energy means inactive, but capable of being called into action. Thus, if a weight be raised, a certain amount of energy is expended in raising it, and so long as the body is supported the energy expended in raising it remains potential in it, but when allowed to fall freely *in vacuo* to the level from which it was raised, the body acquires exactly the amount of energy that was expended in raising it. In the same manner the repulsive force of the molecules of the highly ignited gases into which gunpowder is resolved by ignition may with equal propriety be said to be potential in the unignited powder.

The remarks with which the writer's interpretation of the terms "force" and "energy" have been met by the reviewer may here be appropriately noticed. Quoting the introductory chapter already referred to, he adds: "his doctrine regarding the nature of force has thus no connection with sound philosophy; by force Mr. Brooke evidently means what other advocates of conservation mean by potential energy." Does then sound philosophy consist in the impossible task of agreeing as to the meaning of terms with those who do not agree amongst themselves? Or is sound philosophy monopolised by, and crystallised in the opinions of the reviewer? Sound philosophy would seem to consist rather in assigning appropriate meanings, involving no inconsistency or contradiction, to terms of frequent occurrence in all works on physics.

If the above definitions of force and energy be accepted, it is obvious that the term "force," as used by Grove, Tyndall, and many others, means sometimes force and sometimes energy. Thus, for example, "the conservation of force" becomes a simple truism, for its exercise being a function of matter, force must necessarily be coeval with matter. The reviewer (p. 22) thus quaintly expresses the relations of force, energy, and motion:—"A given motion viewed as a cause is force, while the very same motion thought as an effect is energy." Motion, it is presumed, can mean nothing else than the act of moving; but how the act of moving, whichever way we look at or think of it, can either produce energy or do work, it is difficult to conceive; has this "any connection with sound philosophy"? and does not the reviewer here himself "forget that each thing is itself, and not something else" (p. 22, l. 89).

---

* Faraday, MSS. Croonian Lectures on Matter and Force, by H. Bence Jones, M.D. p. 35.

† Introductory chapter to the Sixth Edition of "The Elements of Natural Philosophy."

The writer would commend to the serious attention of his brother-physicists the above definitions; he would also submit the following definitions of sound, light, and heat, the former of which has, however, been elsewhere declared to be incapable of definition,* as well as by the reviewer (p. 22). Sound may be defined to be the impression produced by certain vibratory movements of matter upon appropriate sensuous organs, including both the receptive and perceptive apparatus. Whether the tympanum be incapable of receiving sonorous vibrations, or whether it vibrate sympathetically while the structures of the internal ear are incapable of appreciating its vibrations, there can be no sound.

And why may not the same definition apply to light and heat? It is, in fact, far from improbable that the perceptions of light and heat may result from the impressions produced by the same identical vibrations on different receptive organs; that of light on the eye and its nervous appendages, that of heat on the organs of common sensation. In common parlance, the terms sound, light, heat, will doubtless continue to be applied indiscriminately to the vibratory motion producing, and to the impressions produced; and to this there can be no objection, provided no advantage be taken of the misnomer to found thereon an assumption of the identity of the proximate cause and the resulting effect.

The reviewer has sought to kill two birds with one stone, and has made a vigorous onslaught against the conservation of energy in general, and the writer in particular, regarding the theory of latent heat; but it may reasonably be questioned how far "sound philosophy" is shown in attempting to convict an author of admitting an insuperable difficulty in the adoption of a given principle by quoting his statement of the difficulty, and coolly suppressing his immediately subsequent explanation of it. He thus quotes the writer†:—"Latent heat has ever been held up as the great stumbling-block of the dynamic theory, because it is impossible to conceive motion to be reduced to a state of quiescence, but remaining still ready to start again into action."

But instead of continuing the quotation thus:—"This, however, is merely a confusion of ideas, the fact being that when any substance passes from the solid to the liquid, or from the liquid to the gaseous form, a certain portion of the impressed heat-force is continuously occupied in overcoming molecular attraction, and thereby effecting the change of form; and this heat cannot be imparted to other bodies so long as it is occupied in maintaining that change," he ventures to state:—"In this we quite agree, and hence we think it unnecessary to give Mr. Brooke's reasons for believing a doctrine which he allows to be inconceivable"!!! Whether "sound philosophy" or not, is this common literary honesty? What Mr. Brooke allows to be inconceivable is obviously not the doctrine itself, but the conclusion drawn from fallacious arguments adduced in opposition to it; for to assign reasons for believing what one allows to be inconceivable would be nothing less than pure and simple evidence of mental alienation.

The fact is that the term "latent heat" is an unfortunate one, and has mystified the reviewer, as well as many others. It ought long ago to have been consigned to the limbo of discarded hypotheses, together with the material or corpuscular theory of heat from which it arose. If heat consisted of material particles, it might be supposed to become latent among the molecules of grosser matter, just as a handful of shot, if dropped into a box full of marbles, would lie hid amongst them, and be lost to sense, and would so remain until shaken out again; but mere vibratory motion cannot be theoretically dealt with in this fashion.

A much better term would be *engaged* or *occupied* heat, for the so-called latent heat is wholly engaged or occupied in maintaining the change—first from the solid to the fluid state, and secondly from the fluid to the gaseous. The facts are very plain; a pound of water at the temperature of 0° C., or the freezing point, mixed with a pound of water at 79° yields two pounds at the mean temperature of 39·5°; but a pound of ice or dry snow at the temperature of 0° mixed a pound of water at 79° yields two pounds of water at 0°, because the 79° of sensible heat in the water are now employed or occupied in maintaining such an amount of vibratory motion in the molecules of the ice, that they are no longer able to obey that polar attraction by which they were previously aggregated together in given directions so as to form crystals (for though not so evident in ice, the crystalline character of snow is notorious), and the heat-energy, being thus occupied in doing work, is incapable of doing any other work, as for example on the organs of sensation, at the same time; on the principle that you cannot "eat your cake, and have it too." The same reasoning applies to the change from the fluid to the gaseous state; but in this case a much larger amount of thermic energy is employed in so far removing the molecules from the sphere of each other's attractions that the balance of their mutual forces is repulsive, and so long as that repulsion is maintained, the dry steam manifests all the properties common to the fixed gases. "Latent" heat, then, when properly understood, ceases to be a "stumbling-block to the dynamic theory of heat."

One finds oneself occasionally brought by circumstances into an unwelcome generalisation. Thus the reviewer, speaking of the supporters of "conservation" in the lump, says (p. 21) "they take it for granted that force is motion and nothing but motion." This the writer entirely and absolutely denies. The reviewer, then immediately preceding his observations on the writer's views, quotes from Prof. Bain that "Inert matter in motion is force under every manifestation." This is plainly an abuse of language, in which the writer, as one of the "they," declines to participate; inertness and force are hardly concomitant ideas, and matter, whether in motion or at rest, is assuredly not force. The term heat-potential adopted by Mr. Rankine is admissible only in relation to heat as previously defined; the thermic energy can hardly be termed potential while it is employed in doing work.

The reviewer (p. 19) quotes, and objects to, the explanation of latent heat offered by Prof. Tait: that while sensible heat is *motion*, latent heat is *position*. The writer must acknowledge his inability to derive any definite idea from this statement of Prof. Tait; he cannot therefore express either assent or dissent.

The writer must plead guilty to having made use in the treatise above referred to, in common with many others, of a phrase which is not strictly accurate, viz., that sound, light, heat, and electricity are modes of motion. It would be more exact to state that they are so many forms of energy, or effects due to matter affected by certain modes of motion.

It is rather surprising that the reviewer should have ventured to dogmatise on the very slender knowledge either of physical facts or hypotheses that he evidently possesses. Thus he states (p. 33, note) regarding the investigations of Dr. Joule:—

"By means of machinery a weight of 772 lbs. is made to turn a small paddle-wheel placed in one pound of water. Dr. Joule found that the descent of the weight with a given velocity through one foot raised the temperature of the water exactly 1° F." Now this small sentence contains a curious concatenation of errors. Dr. Joule never employed a weight of 772 lbs., for the weights he employed were all under 30 lbs.; he never employed the energy acquired by his descending weights in stirring an exact pound of water, for the stirring vessel was not constructed to hold that particular quantity; neither did he find that the descent of the weight with a given velocity raised the water exactly 1° F., for the velocity of descent has nothing whatever to do with the result; since precisely the same amount of energy is acquired by a body in descending through the space of one foot by the action of gravitation, whether one minute or one month were occupied in the descent. For the sake of convenience, and in order to avoid a source of error, it was desirable only that the weights should descend slowly, and with a tolerably uniform velocity.

---

* "A logical definition of sound is impossible." Dr. M'Cann on "Force and its Manifestations," a paper lately read before the Victoria Institute.

† Elements of Natural Philosophy (p. 786).

So much for the reviewer's knowledge of facts; now as to his knowledge of theories. He writes (p. 31):—

"The supporters of the doctrine of the indestructibility (? conservation) of energy have adopted a method the reverse of scientific. They start with the assumption of perpetual motion by means of transformation. In order to make facts fit their hypothesis, they take for granted that heat, light, electricity, and magnetism, are modes of motion, but not requiring a material basis to account for their phenomena. Some, however, seem to be aware that motion of necessity implies something moving, and that this something must be matter in some of its forms, and that consequently it is a great mistake to suppose that the dynamical theory is inconsistent with the materiality of heat. Finding that they have been a little too hasty in getting rid of the old imponderables, they are now quietly bringing them back under a new name, hoping, doubtless, that their few remaining friends may not be able to recognise them. Instead of the 'imponderables' we now have the 'luminiferous ether' which fills stellar space, and even permeates all the grosser forms of material existence."

This sentence expresses a gross misrepresentation of the course of philosophic thought. Can the reviewer point out a single physicist who for a moment doubts that "motion of necessity implies something moving, and that this something must be matter in some of its forms;" it is an axiom that not "some" but all must obviously admit. When it was supposed that light and heat consisted of material particles projected with immense velocity from their radiant sources, and that electricity and magnetism were "fluids" travelling with similar velocity, it might be assumed that inter-stellar space is an absolute vacuum; but when the progress of physical knowledge developed phenomena which were partially or wholly incapable of explanation on this hypothesis (such as diffraction and interference, and subsequently those of the polarisation of light and heat); but which became perfectly intelligble on the hypothesis that these forms of energy consisted in vibratory molecular motion transmitted with the same great velocity, the existence of a highly attentuated and elastic medium as the denizen of infinite space, became a necessary part of the theory; and this, in unavoidable ignorance of its precise nature, was termed "ether." Hence, in direct opposition to the reviewer's statement, physicists take for granted that "light, heat, electricity, and magnetism" do require a "material basis" (that is, matter as a means of their transmission) "to account for their phenomena;" and his dictum about physicists getting rid of the old imponderables, and now bringing them back again, is unmitigated nonsense.

The attribute of imponderability has been ascribed to the hypothetical substance "ether" by many physicists who hold that it monopolises the property of transmitting the waves of light and heat, and is therefore interstitially deposited in all kinds of matter. The writer is, however, more inclined to believe, with Mr. Justice Grove, that all kinds of matter are susceptible of these vibratory motions, and hence that the hypothesis of *intersticial* ether is gratuitous; his reasons for such opinion being elsewhere in print, need not be here repeated.[*]

It may be further suggested that for all that is positively known to the contrary, all kinds of matter may possibly be susceptible of a fourth state or condition, which may be termed the "ethereal," and which in tenuity and elasticity may be as far beyond the gaseous, as the gaseous is beyond the fluid state; possessing also the mechanical properties of a jelly, rather than those of a gas.

The writer can hardly be expected to take up the cudgels for others against the reviewer; but in concluding the remarks on this point, it may be observed regarding the reviewer's emphatic denunciation of an alleged descrepancy between Grove and Tyndall, that the discrepancy exists only in his own misunderstanding the quotation from the "Correlation of Physical Forces," viz., that "it requires no great stretch of imagination to conceive light and electricity as motions, and not as things moving," in which the writer clearly contrasts the undulatory and corpuscular theories, as commonly understood. Enough has now been advanced to show that the reviewer need not look very far from home for a conspicuous example of that which he has so freely attributed to the unhappy physicists, namely that they do but "darken counsel by words without knowledge" (p. 23).

The equivalence of dynamic and thermic energy is the only one that has as yet been determined quantitatively. He must be a bold man who denies that the sun shines at noonday; and scarcely less audacious is the assertion of the reviewer that the experiments of Dr. Joule do not confirm this equivalence. Dr. Joule conducted four distinct series of experiments, three series on the amount of thermic energy produced by molecular friction in stirring respectively water, oil, and mercury; the fourth, on that produced by the friction of two iron surfaces against each other. The four numerical results accorded very nearly, and after assigning to each result its weight, according to its estimated liability to error, he deduced the mean value of 772 foot-pounds as the dynamic equivalent of thermic energy.[†] In the metrical system, in which the units of quantity are one kilogramme, one metre, and one degree in the centigrade scale, the above equivalent is represented by 424 dynamic units, which, for brevity's sake, we may as well agree with the French in calling "dynams."

The reviewer, in ignorance probably of the amount of labour bestowed on this subject, seems to imagine that by ignoring Dr. Joule's results, he has demolished the basis of thermodynamics; but if so, he is grievously mistaken. It is a remarkable and unprecedented confirmation of this theory, that the numerical results arrived at by three distinct methods of investigation, in the hands of as many independent physicists, should be found to agree within very narrow limits of error.

It has been found by experiment that a less amount of heat is required to raise a gas maintained at a constant *volume* one degree of temperature, than when the gas is allowed to expand under a constant *pressure*. Suppose, for example, that the gas be enclosed in a vertical cylinder under a piston of 100 square inches area, the atmospheric pressure on this piston will be 1,500 lbs., and the raising this piston is equivalent to raising a weight of that amount. Dr. J. R. Mayer, assuming that the difference in the quantities of heat in the two cases above mentioned is equivalent to the work done by the expanding gas, proceeds to determine the numerical value of these equivalent quantities. Taking the specific heat of air to be 0·267, as at that time determined by the observations of De la Roche and Berard, he found the dynamic equivalent of an unit of thermic energy to be 367 dynams. But if, in the calculation of this number, the more careful and accurate subsequent determination of the specific heat of air by Regnault be substituted, namely, 0·2375, the result gives as the equivalent 426 dynams; a result almost identical with that of Dr. Joule, but based on purely theoretical considerations.

---

[*] Lecture on Force and Energy, delivered at the Royal Institution, *Medical Times and Gazette*, July 8, 1871.

[†] For the sake of those readers who are not already familiar with this subject, we may state that a foot-pound is the amount of energy acquired by a weight of one pound in descending through the vertical space of one foot, or, in other words, the amount necessary to raise one pound one foot; and the numerical equivalent here given means that 772 dynamic units are equivalent to the amount of thermic energy required to raise the temperature of one pound of pure water, at or about the mean temperature of the air, one degree of Fahrenheit's scale.

Subsequently M. G. A. Hirn* pursued a course of observations exactly the reverse of that of Dr. Joule, namely, to determine the amount of heat converted into work in the steam-engine. Taking it as an axiom, which he showed to be in strict accordance with analytical investigations, that the difference between the heat existing in the steam as it enters the cylinder and that remaining in it after its exit, must be the thermic equivalent of the work done in and by the engine (which difference in the best constructed engines amounts to about 5 per cent. of the total heat due to the combustion of the fuel), he determined the value of one thermic unit to be 425 dynams; a remarkable result, and intermediate between those previously obtained by Mayer and Joule. In the face of such overwhelming concurrent evidence will even the reviewer be still bold enough to assert that the conservation of energy is a myth?

The principle of the dissipation of energy, as a corollary to that of its conservation, is of course equally ignored by the reviewer; but as his remarks on that point have no relation to anything beyond his own inner consciousness, it must here suffice to give a familiar illustration both of the conservation and the dissipation of energy, in the action of the rifle-ball. This reaches the target with less velocity, and consequently with less energy, that it possessed on leaving the muzzle; a portion of its energy has been expended in producing heat by friction against the particles of air between which it passes, which is dispersed through the surrounding atmosphere, and thus becomes *dissipated*. On reaching the target the progressive motion of the mass is arrested, and converted into molecular motion, which is cognisable only as heat, by which the mass is reduced to the fluid state, and splashes of molten metal are scattered in all directions. These again impart their heat partly to the air through which they pass, partly by radiation into space, and partly to the ground on which they fall; and thus the whole energy of the ball becomes dissipated. An analogous explanation will apply to all other cases of the dissipation of energy; thus the principle may be indefinitely illustrated, but it is hoped that it has now been sufficiently established that the conservation of energy is a fact, not a "heresy of science," the reviewer's opinions to the contrary notwithstanding, for

> A man convinced against his will
> Is of the same opinion still.

Want of space forbids the consideration of the larger cosmical relations of the conservation of energy to both organic and inorganic existence; one proposition only shall be alluded to as having been by some writers rather overstrained, viz., that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity. This may be taken as a postulate, and is probably true, but it is a proposition that is equally incapable of proof or of disproof, because the amount of potential energy in a body can be determined only by its development into actual energy, and cannot therefore be predicated. For example, two stones of equal weight lie one at the bottom of a well, the other on the ground at its edge; both are raised and placed side by side on the top of the windlass frame, much more work has been done on one than on the other, but can any one predicate in which of the two the greater amount of potential energy resides? Or is there any conceivable difference in the amount of energy acquired by either, otherwise than as dependent on its descending through the greater or the lesser distance?

It may, in conclusion, be remarked with much regret that the principle of the conservation of energy has by some been misapplied in a fruitless endeavour to supersede the necessity of a creative intelligence. To the mind of the writer, and, it is earnestly hoped, to that of most of his readers, the indisputable establishment of this principle conveys only a more exalted idea of that infinite wisdom by which the perpetually recurring transformations and interchanges, not only of the materials, but also of the powers, of Nature are rendered subservient to predetermined laws, which govern the comfort and welfare of all created beings.

CHARLES BROOKE

SPECTROSCOPIC NOTES*

ALTHOUGH P. Secchi and others have recently published descriptions of the different varieties of solar prominences which have been observed, well illustrating the many forms in which these outbursts from the sun's chromosphere occur, a careful record of such disturbances as may be out of the more common order may in the end assist us to a further knowledge on this subject.

While looking for bright lines near F in the spectrum of a low prominence situated 25° north of east, at 11.35 A.M. (April 2), my attention was called to what appeared to me a sudden displacement of the F line toward the violet end of the spectrum.

I immediately brought the C line into the field of view, in order to discover if any change was taking place in the form of the prominence, but was wholly unprepared for the spectacle which met my eye. Upon widening the slit I found that where only twenty minutes before there had been a comparatively low mass of prominence matter not exceeding 50″ in height, and remarkable only for the brightness of a jet issuing from the southern portion which was but slightly raised above the chromosphere, there had been an eruption of matter on a grand scale.

Fig. 1 represents the prominence as first seen at 11.15 A.M. At 11.35 A.M. the northern portion had entirely disappeared, and from the low mound, at the point where the jet mentioned above had been seen, an eruption had taken place far exceeding anything I have ever witnessed. Far above the chromosphere the air was filled with long wisps of glowing hydrogen, ranging from 20″ to 50″ in length, with the appearance of having been ejected in quick succession. Above them floated detached masses, in the form of thin fleecy clouds, and the highest point reached by these was fully five minutes of arc, or about 135,000 miles above the sun. In the mound appeared a low, sharp horn, exceeding in brightness any other portion of the prominence. The grandeur of the eruption lasted but a few minutes, gradually fading in brightness and diminishing in size.

Fig. 2 gives a correct idea of the general form and extent of the prominence as seen at 12 M.

When next observed at 1.40 P.M., nothing was left but a small cloud mass of about the height of the prominence when first seen, at 11.15 A.M. To the north of this, however, were two very small horns, of great brightness. It has generally been noticed that the appearance of these bright points in the chromosphere is a forerunner of increased activity. The forces at work beneath were only gathering strength for a final outburst, which, if not so great in extent, proved to be equally magnificent. I had not long to wait; for after a few minutes spent in examining the F line, just as the clock was striking the hour, I again brought C into the field, and found myself only in

---

* Cosmos, v. 22, 1863.

* Reprinted from advance sheets of the Journal of the Franklin Institute furnished by the Editor.